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## SELECTIVITY IN ESP EXPERIMENTS

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I AM going to discuss a point raised by Dr Thouless in his contribution to the symposium on a programme for the next ten years of research which appeared in the *Journal of Parapsychology*.<sup>1</sup> Thouless wrote: 'I suggest investigation of the conditions of singularization. Positive results in ESP or PK experiments require that the subject should have reacted to (or acted on) one particular object or set of objects and not others of the same kind. The problem of how it happens that the experimental object is thus picked out seems to have attracted little attention except for the restricted case of what is called "psychometry."' In discussing this problem I shall use the word 'selectivity' instead of 'singularization', partly because it is more familiar, but also because it seems to me more neutral with respect to the question whether the role of the subject is active or passive. (We speak not only of people actively selecting things, but also of machines, e.g. radio sets, being passively selective.) I would state the problem, then, as being to ascertain the conditions which influence selectivity. I shall only consider the problem as it arises in ESP experiments. The problem appears much less conspicuous in PK experiments in which subjects have usually been able to look at the objects they were trying to influence.

Now one—and perhaps the most important—virtue of Carington's theory of telepathy is that it offers us an explanation of the subjects' selectivity. I know of no other theory which does this. Physicalist theories are notoriously incapable of explaining why a subject should respond to a radiation emanating from a certain brain (or pack of cards) rather than that from any other brain (or pack) within the same distance. On the other hand, to invoke

<sup>1</sup> 'A Program of Parapsychology', *J. Parapsychol.*, vol. 12, no. 2 (June 1948) p. 117.

'rapport' between subject and agent as a necessary condition of telepathy is, in the absence of a theory, merely to give a label to one of the facts which requires explanation.

As is well known, Carington explains telepathy by supposing (1) that an idea of the target object becomes associated in the mind of the agent (and/or that of an experimenter) with a second idea; (2) that this second idea (which he calls a K-idea) occurs, as a result of normal processes, in the mind of the subject; (3) that these two events tend to produce jointly a third event—the subject's thinking of the target object. On this theory, the factor which makes the subject select (or selective to) the target is the K-idea in the mind of the agent (or experimenter). It is difficult to understand how an idea could perform this role if it were one which was simultaneously being entertained by people not participating in the experiment; but Carington pointed out that there will always be one idea which will be entertained only by those who are participating, or, if entertained by non-participants, will not be so important to them—namely the idea of 'this experiment'. We must admit that Carington's theory does not take us as far as we would wish, for we still cannot understand how one person's associations could influence another person's experience in the conditions in question—we do not know whether any mediating processes need occur, nor what their nature would be. Nevertheless, our starting point in investigating selectivity ought surely to be an attempt to submit Carington's theory to the test of experiment. To do this I should suggest that we carry out some such experiments as the following:

(i) Using a subject who, like Mrs Stewart, has obtained good results with several agents, we could employ two or more of these agents working in opposition (i.e. attending simultaneously to different target-cards). At the start of each series each agent would be given a different code-name and/or picture to be associated with the ensuing targets, and one of these code-names would be selected at random and given to the subject. We should thus be deliberately providing a K-idea to supplement the idea of 'this experiment'. Precautions would be taken to ensure (a) that neither the subject nor the experimenter controlling the subject received any sensory clues regarding the other code-names in use and the allocation of code-names to agents; and (b) that neither the agents nor the experimenter(s) controlling the agents received any sensory clues regarding the code-name given to the subject. If we found a regular tendency for the subject to get significant scores only on the cards being thought of by the agent with the corresponding code-names, this would support Carington's theory.

(ii) Alternatively, or in addition, we could employ only one

agent, who would work simultaneously with two series of targets, each series being associated by the agent with a different code-name, one of which had been given to the subject.

It will be noticed that the former experiment is very similar in design to one which Dr Soal performed recently with Mrs Stewart. (This is described in § 33 of his Myers Lecture *The Experimental Situation in Psychical Research*.) Soal carried out two series, each of 400 trials, with two agents working in opposition. In the first 200 trials of each series Mrs Stewart was informed that there were two agents, but not that they were working in opposition. Dr Soal tells us that 'the pooled results on the 400 trials show that with neither agent was her score significant'. In the second 200 trials of each series, Mrs Stewart was told the name of one of the two agents and was given to understand that this agent would be working alone. In this case, the pooled results show a highly significant score on the targets of this agent with a chance score on the targets of the other agent. Now these results are just what Carington's theory would predict, but the support which they give to this theory is unnecessarily weak. (We must of course remember that Soal was not trying to test Carington's theory.)

Let us consider Soal's conclusion—that the results of the above experiments 'illustrate the importance of conscious orientation in telepathy experiments'. This statement is somewhat vague, for what is meant by 'conscious orientation'? Presumably Soal meant by this the subject's adjusting herself to the experiment by imagining the agent at work. Conscious orientation, in this sense, would involve her possessing knowledge of two kinds: (a) being acquainted with the person who acts as agent, i.e. having, by normal social intercourse, established a personal relationship with this person, or, at least, being familiar with the appearance of this person;<sup>1</sup> (b) knowing that it is this person (identified by name or other verbal description) who is acting as agent. Was knowledge of either of these kinds necessary or favourable to Mrs Stewart's success? Soal's above-mentioned experiments do not enable us to answer this question.<sup>2</sup> The main interest of Carington's theory,

<sup>1</sup> The subject might of course be *familiar with the appearance* of the person who acts as agent through having seen a photograph, without having seen this person 'in the flesh'. Whether the former is an adequate substitute for the latter is a matter for experiment. To simplify exposition I shall ignore this complication in what follows.

<sup>2</sup> Since this paper was written, a report of some long-distance experiments with Mrs Stewart has been published by Bateman and Soal in the *S.P.R. Journal* (Vol. XXXV, No. 659). The results are highly relevant to the question I am considering here; for in the Cambridge-Richmond series, in which she was not acquainted with the agents, Mrs Stewart failed completely, whereas in the London-Antwerp series, in which both

in this context, is that it suggests that such knowledge may not be relevant to a subject's selectivity. When Soal told Mrs Stewart the name of one of the agents (suppose it was 'John Brown') this name *may* have been functioning, solely as a K-idea, in Carington's sense; that is to say the name 'John Brown' may have made Mrs Stewart select (selective to) the targets being thought of by John Brown solely because these targets were associated in the mind of John Brown with his ideas of himself and of his own name. Mrs Stewart's being acquainted with this person called 'John Brown' and/or her knowing that it was this person who was acting as agent may have been irrelevant to her success.

It seems important to try to ascertain the relevance of the factors to which Soal seems to have been referring when he spoke of 'conscious orientation', for it seems to be commonly taken for granted that these factors are necessary, or at any rate favourable conditions of telepathy. I am not sure that I understand what psychical researchers mean by 'rapport', but I suspect that some, when they use this term, have in mind a theory to explain telepathy—a vague theory which might be expressed by saying that 'rapport' means 'direct psychic contact' between two (or more) persons whose bodies are not manifested to each other's senses. On this view, it would be natural to assume (i) that it is a necessary condition of the establishment of such rapport that the persons in question should be previously acquainted with each other, and/or (ii) that the more intimately they are acquainted the easier it will be to establish such rapport. Adopting Carington's theory would not imply that we should drop the word 'rapport'; we could still say that rapport is a necessary condition of telepathy, but rapport would become a much more precise concept, to be defined not vaguely in terms of 'direct psychic contact'—which, incidentally, suggests that it is a matter of all or none—but rather in terms of two (or more) minds entertaining the same (or similar) ideas, which is a matter of degree. On this view, a high degree of rapport might hold between people who were not acquainted with each other, yet the minimum degree of rapport—two people 'sharing' a single idea—might be a sufficient condition of telepathy, if, for example, the idea in question was important to each of them and was not 'shared' by anyone else. One reason why Carington's theory deserves serious attention is that there are

of the agents were her close friends, Mrs Stewart was highly successful. This strongly suggests that, for Mrs Stewart, being acquainted with the agent is a necessary condition of success. Unfortunately there was another factor which may, the experimenters tell us, have adversely affected the Cambridge-Richmond results, namely inadequate synchronisation of the stop-watches.

records of spontaneous cases in which 'conscious orientation' seems to have played no part whatsoever. I have in mind the type of case where a subject receives an impression of an accident involving no persons with whom the subject was acquainted or connected (e.g. the case discussed by Mr Tyrrell in *Proc. S.P.R.*, Vol. XLVIII, Part 173, pp. 89-91). In such cases I can think of no explanation of the subjects' selectivity except that provided by Carington's theory.

We require, then, to test the relevance to the subject's selectivity of the following factors :

(x) the subject being acquainted with the person(s) who act(s) as agent(s) ;

(y) the subject knowing (by name or other description) the identity of the person(s) who act(s) as agent(s).

I suggest, therefore, that the experiments which I proposed earlier be performed under each of the following conditions :

(1) with  $x$  and  $y$  both fulfilled.

(2) with  $x$  but not  $y$  fulfilled.

(3) with neither  $x$  nor  $y$  fulfilled.

We might for completeness try variation (4) i.e. with  $y$  but not  $x$  fulfilled. It is, however, difficult to see how the name (or a description) of an agent with whom the subject was not acquainted could be relevant *except* as a K-idea, in which case the only difference between conditions (3) and (4) would be that in the former case two K-ideas would be used, in the latter three. (I am counting the idea of 'this experiment' as a K-idea, as well as the code-name.) I should not expect conditions (3) and (4) to yield different results, but if they did it might be worth investigating whether the description of the agent which was given to the subject had to be a unique description (i.e. applicable to no one else) in order to affect the results. (In some cases a person's name is a unique description—Whately Carington's probably was, and this point may be relevant in applying his theory to his own experiments with drawings in which many of the subjects were not acquainted with him.)

One further factor which might with profit be varied in the above experiments is the subject's knowing the location of the agent(s). We already have one experiment whose results suggest that this factor is irrelevant (reported by McMahan and Lauer in the *Journal of Parapsychology*, Vol. 12, No. 1). This result requires confirmation.<sup>1</sup>

<sup>1</sup> Some further confirmation has now been provided in the long-distance experiments with Mrs Stewart. (Bateman and Soal, *op. cit.*, pp. 266-7.)

We may learn from such experiments that for different subjects different sets of conditions are necessary for or favourable to selectivity. If, however, we found any subjects able to select the appropriate target under conditions (3), this would be an important conclusion and one which would call for a thorough testing of Carington's theory in all possible respects. One of the next steps would be to test the sub-laws of the association theory—the laws of recency and frequency. Carington found quantitative evidence that these sub-laws had been fulfilled in some of his experiments with drawings, and this should be checked in other experimental situations. In connexion with the law of frequency, there would of course be two different factors to be investigated :

(a) the effect of repeating the conjunction of the idea of the target and the K-idea in the *same* agent.

(b) the effect of increasing the number of agents in whose minds these ideas are associated.

I shall now consider a possible criticism. It might be said that any adequate theory of selectivity must be applicable to clairvoyance as well as telepathy, whereas Carington's theory applies only to telepathy. This criticism raises two issues. The first is whether it is possible to discriminate between 'pure clairvoyance' and 'pure telepathy'. The second is whether Carington's theory could be applied to explain selectivity in cases of 'pure clairvoyance'. I shall briefly consider each of these issues.

Regarding the first issue, I am inclined to think that in their recent attempts to isolate 'pure telepathy' and 'pure clairvoyance' experimenters may have been pursuing a will-o'-the-wisp. At any rate, there is one difficulty which they do not yet seem to have considered. It is generally (and rightly) agreed that if, in a telepathy experiment, an agent or experimenter at any time mentions *aloud* the target-object or a code from which this might be inferred, this utterance, being a physical event, constitutes a potential object for the subject's clairvoyant faculty. It seems to be taken for granted that if an agent or experimenter merely thinks of the target or a code without mentioning it *aloud*, there is no physical event constituting a potential object for clairvoyance. This assumption seems to be unwarranted in view of the well-known fact that our thinking is usually (some psychologists say always) accompanied by certain physical events, notably the small movements of one's tongue and larynx which occur as one silently talks to oneself, and which systematically resemble the larger movements which occur in these organs when one utters the same words aloud. To admit this is not to adopt the (to my mind unacceptable) view which identifies thinking with sub-vocal

speech. But if we grant that sub-vocal speech movements are normal unconscious concomitants of our thinking, how are we to eliminate these as possible<sup>1</sup> objects of clairvoyance? In the recent 'pure telepathy' experiments in which the agent has employed a code relating target-faces to numbers, the agent has presumably, in the process of memorising the code, run over it several times in unspoken words. This difficulty may be surmountable—e.g. if the agent who uses the code only speaks a language not understood by the subject—but it is not my present purpose to explore such avenues. It is at any rate certain that attempts to isolate telepathy and clairvoyance involve us in some very complicated problems. We need not, however, postpone investigation of selectivity until each of these problems is solved or found to be insoluble. We may be able to advance a long way in understanding selectivity by means of experiments whose design is relatively simple. In the 'telepathy' experiments which I proposed above, I think we should not at present bother about trying to eliminate all possibilities of clairvoyance, but should employ the GESP<sup>2</sup> method.

Regarding the second issue, it does not follow that because Carington's theory was devised to explain telepathy, it could not be used to explain selectivity in cases of clairvoyance. The following questions can be distinguished and may require different answers :

(a) What makes the subject select (selective to) the object which an experimenter has decreed to be the target, e.g. this pack rather than another?

(b) How does a subject succeed to a significant extent in identifying the nature of the target-object, e.g. the order of the target pack?

(These questions could be distinguished, though it does not seem necessary to do so, in dealing with the facts usually attributed to telepathy.) When these questions are distinguished in considering clairvoyance, the former can be answered by saying that the subject

<sup>1</sup> Professor H. H. Price has pointed out to me that lip-reading has to be learned and is difficult to learn, and that the sort of clairvoyant larynx-reading which I am postulating would be much more difficult to learn. This must be granted. All that I am claiming is that since this sort of larynx-reading cannot be ruled out as impossible, this undermines the view that recent experiments have re-established conclusively the occurrence of 'pure telepathy'.

<sup>2</sup> GESP stands for 'General Extrasensory Perception', and is applied to experiments in which the target cards lie successively before the agent's eyes. Thus the subject may get his information either by clairvoyance from the cards themselves or by telepathy from the agent's perception of the cards.

selects the pack in question because it is (most strongly) associated in the mind of an experimenter with his idea of 'this experiment'.<sup>1</sup>

Now obviously we can test the applicability of Carington's theory to the selectivity of subjects under conditions which have usually been called clairvoyant. Let us not bother at this stage about excluding all possibilities of telepathy, but employ a method like DT<sup>2</sup> calling. We can arrange an experiment in which the experimenter controlling the target (E C T, as we may call him) uses two or three alternative target packs, each associated by him (and only by him) with a certain code-name, while the subject, located elsewhere, is given one of these code-names. If such experiments yielded the predicted results, there are several factors which it would be interesting to vary one at a time :

- (1) the subject being acquainted with E C T.
- (2) the subject knowing by description the identity of E C T.
- (3) the subject knowing the location of E C T.
- (4) E C T knowing the location of the target pack corresponding to each code-name.

Since the above was written, an important article by Soal and Bateman has appeared in the *Journal of Parapsychology* (September 1950) giving a detailed report of all the experiments performed with Mrs Stewart up to July 1949 in which two or more agents have been used in opposition or in conjunction. Much of this material has not been previously published, and the results display some new and interesting features, notably :

(i) When Mrs Stewart has been working successfully with agent No. 1, and agent No. 2 is introduced acting in opposition to No. 1, Mrs Stewart continues to work with No. 1 and 'ignores' No. 2. This feature recurred consistently in a series of experiments in which Mrs Stewart was fully informed as to the identity of the agents and their roles and in which agent No. 2 was a person with whom Mrs Stewart had previously worked very successfully.

<sup>1</sup> Professor Price has pointed out to me that what I say here would apply only in the experimental situation, for in a case of spontaneous clairvoyance there would be no experimenter decreeing that a certain object is the target. I should, however, recommend that in examining spontaneous cases of apparently pure clairvoyance we should look for a person who might be performing this function of the experimenter, i.e. a person in whose mind the clairvoyantly perceived object might be associated with a K-idea. Could we be certain in any given case that no such person existed?

<sup>2</sup> DT stands for 'Down Through', and is applied to experiments in which the subject guesses the order of the pack from top to bottom, the order not being observed by anyone until the subject's guesses are completed. This method does not eliminate precognitive telepathy.

(ii) It appears that when several agents are used in conjunction (i.e. each attending simultaneously to the same target), Mrs Stewart works with only one of these agents and 'ignores' the others. This very interesting conclusion is an inference based on two types of experiments:

(a) in which a single agent is used for the first 200 calls and a second agent acts in conjunction with the former for the second 200 calls. There were three experiments of this design. In two of them Mrs Stewart got a significant score for the first 200 calls, and the conjunction score for the other 200 was at approximately the same level. In the third case, the person who was used as agent No. 1 appears to have been a bad agent (independent evidence of this was obtained). The scoring for the first 200 calls was at chance-level, but was significant for the second 200.

(b) in which the same two (or three) agents are used throughout a block of 400 calls, their roles alternating every 50 calls between opposition and conjunction. If we take the first four experiments of this type, we find that when the agents were in opposition Mrs Stewart obtained in each case a significant score with one agent and chance scores with the other(s). If we take the pooled results of these four experiments, we find that the conjunction score and the highest opposition score are both highly significant. The conjunction score is in fact the lower (209/800 against 231/800)—*but there is no significant difference between these scores* ( $P=0.16$ ).

One cannot do justice in such a brief summary to the material contained in this article. However, these latest results do not, so far as the writer can see, provide any grounds for choosing between the two theories of 'rapport' which have been discussed earlier—the theory of 'conscious orientation' on the part of the subject, and the theory suggested by Carington. The disclosure of these new facts does, however, accentuate the problem discussed in this paper, and provides a further challenge to us to try to ascertain the factors which influence selectivity in psi-phenomena.

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## PERROTT STUDENTSHIP IN PSYCHICAL RESEARCH AT TRINITY COLLEGE, CAMBRIDGE

The following communication has been received from the Electors to the Perrott Studentship in Psychical Research :

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined, for the purpose of the Studentship, as ' the investigation of mental or physical phenomena which seem *prima facie* to suggest (a) the existence of supernormal powers of cognition or action in human beings in their present life, or (b) the persistence of the human mind after bodily death '.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating, in consultation with a Supervisor to be appointed by the Electors, some problem in Psychical Research. The Student shall not, during his tenure of the Studentship, engage in any other occupation to such an extent as would in the opinion of the Electors interfere with his course of research. Residence in Cambridge is not required.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake.

Applications from candidates should be sent to THE SECRETARY, PERROTT STUDENTSHIP ELECTORS, TRINITY COLLEGE, CAMBRIDGE, not later than 30 April 1951. Intending candidates should write to the Secretary for further details before applying.

The election to the Studentship will take place in the Easter Term of 1951, and, if a candidate be elected, his tenure will begin at Michaelmas following the election.

The Perrott Studentship was established in 1940 out of a bequest left to Trinity College, Cambridge, for that purpose by Frank Duerdin Perrott as a memorial to Frederic W. H. Myers. Myers, like Edmund Gurney and Henry Sidgwick, also founders of the Society, was a Fellow of Trinity.

The Studentship was first held by the late Whately Carington, who began his tenure in the Michaelmas Term of 1940 and who was engaged in experiments on the paranormal cognition of drawings. The Studentship was next held by Dr S. G. Soal for a year from Michaelmas 1948, during which time he carried out ESP research with his subject Mrs Gloria Stewart.

## REVIEWS

PSYCHICAL RESEARCH, ETHICS AND LOGIC. Supplementary Volume XXIV to *Proceedings* of the Aristotelian Society. London, Harrison & Sons, 1950. 231 pp. 21s.

At the annual joint meeting of the Mind Association and the Aristotelian Society, held at Bristol in July 1950, there was a symposium on the relevance of psychical research to philosophy. The three contributors to it were Mrs M. Kneale, Mr R. Robinson, and Mr C. W. K. Mundle. Their papers will be found on pp. 173-231 of the present volume. Before discussing them, it may be well to point out that during the last twenty years there has been a change in the attitude of professional philosophers towards their own subject. For good or ill, many of them now think that the traditional problems of philosophy are in the end problems about language, and are to be solved (or else shown to be nonsensical) by methods of linguistic analysis. This linguistic conception of philosophy has not yet spread very far beyond professional circles, and may not be familiar to all readers of the *Journal*.

In the first paper, Mrs Kneale argues that even if we accept the linguistic conception of philosophy, the facts discovered by psychical researchers are still relevant to philosophical problems; what they show is that common sense and scientific terminology are not, in all respects, adequate for the description and explanation of all known empirical facts (p. 176). Thus the physical phenomena of psychical research call for some revision of the terminology of physics, as the phenomena of magnetism did long ago. Again, the mental phenomena show that a purely Behaviouristic terminology is inadequate for describing all occurrences in human beings. When we consider telepathy, we can see that an 'inner life' terminology is needed as well. PK shows us that the old problem of the relation of mind and body is not so dead as Behaviourists think; it compels us to enquire into the relation between the 'inner life' terminology and the 'material object' terminology. It follows that the hypothesis of survival is not meaningless, as an over-indulgence in Behaviouristic terminology might lead us to suppose. But in all these cases, Mrs Kneale thinks, the chief service of psychical research to philosophy is to save us from 'forgetting the obvious'. Thus, it ought to have been obvious to anyone who reflects on mental images, or hallucinations, or eidetic images, or voluntary action, that an inner life terminology is needed in any case. Psychical research mainly

provides an *a fortiori* argument for something we ought to have known already.

I think that Mrs Kneale, in her laudable endeavour to conciliate the Linguistic Analyst, has over-stated her case. Can this be the only lesson which the phenomena of clairvoyance have for philosophers? Perhaps—if clairvoyance were at all analogous to ordinary sense-perception. But surely it is not. (Mrs Kneale merely mentions clairvoyance in passing and does not discuss it.) And what of precognition? Here again Mrs Kneale takes the same over-modest line. She points out that precognition is not foreknowledge, but only fore-imaging. She thinks, or at least hopes, that no very radical revision of the conceptions of Time and Causation will be needed; and she is sure that the phenomena of precognition give no support to mystical doctrines or to theories about 'the nothingness of Time'. Even so, can it really be maintained that precognition merely provides us with an *a fortiori* argument for accepting a terminology which we ought to have accepted in any case? On the contrary, it seems to break the rules of *all* the existing terminologies, physical or psychological, introspective or Behaviouristic. It is not altogether surprising that Mr Robinson accuses Mrs Kneale of 'damning the importance of psychical research for philosophy with faint praise'.

Mr Robinson himself, however, thinks that even this faint praise is unjustified. If the linguistic conception of philosophy is accepted, he can find only one thing in the literature of psychical research which could instruct a philosopher; and that is the terminology used by psychical researchers themselves. For example, when one calls a coincidence a case of telepathy one is not explaining it; one is denying the possibility of explaining it. According to Mr Robinson, the whole point of a phrase like 'ESP' is to denote a process in which information is acquired by no *means* at all. Again, Mrs Kneale does not explain precognition by calling it 'fore-imaging'; she is only telling us that there is *no* explanation of the fact that Mr A. had an image resembling X before X actually occurred. And supposing we did some day discover the means by which telepathy occurs, most psychical researchers would cease to be interested in telepathy. For it is only 'meansless' cognition which is relevant to the metaphysical and theological theories which most of them wish to maintain.

But what if we reject the purely linguistic conception of philosophy? (Mr Robinson, himself, I think does wish to reject it.) Then he is willing to admit that psychical research is relevant to philosophy in two important ways. First, if supernormal cognition does really occur, we shall have to conclude that Empiricism is false. Empiricism tells us that no synthetic proposition is to

be accepted unless it is supported by the evidence of the senses ; and in ESP, if it does really occur, we are acquiring factual information *without* the evidence of the senses. (As Mr Mundle points out later, it would be very odd if Empiricism could be refuted by empirical facts. Mr Robinson's definition of Empiricism is surely too narrow. In any case, the classical Empiricists also laid great stress on the evidence of introspection. Mr Robinson seems to have forgotten the importance which Hume attached to 'impression of reflection', i.e. introspective data.)

Secondly, Mr Robinson also admits that psychical research is relevant to what he calls 'the theory of man'. In his view, we have to choose between two philosophical theories of human nature: on the one hand, the Platonic theory, i.e. the dualistic interaction theory, which regards mind and body as two distinct entities ; on the other, the Aristotelian theory ('the soul is the form of the body') whose modern form is philosophical Behaviourism. By the end of the nineteenth century, it seemed that the question was settled for good and all. The biological sciences had apparently shown that Plato was wrong and Aristotle right, to the confusion of the theologians and other religious-minded persons. But then it occurred to the Platonic party that the scientists might be defeated by their own weapons—by appeal to empirical facts. Their plan was to re-establish the Platonic theory of human nature, not by theological or metaphysical arguments, but by scientific ones. And that, according to Mr Robinson, is what psychical research consists of. Accordingly, he is not surprised to find that psychical researchers attach so much importance to 'the attempt to communicate with men who are not at the time attached to any specimen of *homo sapiens*' (p. 202).

Mr Robinson admits that the psychical researchers have succeeded in collecting a number of 'queer coincidences'. But he still adheres to the Aristotelian theory of human nature. Is this irrational? He claims that it is not. No one can be expected to explain all the queer coincidences which happen from time to time. And the psychical researchers themselves cannot explain them ; to label them 'telepathy', 'precognition', etc., is not to explain them. If one accepted the Platonic theory of human nature on such grounds, one would be falling into the fallacy called the argument from ignorance—'this proposition must be true because we do not know for certain that it is false'.

I cannot but think that here Mr Robinson proves too much. If he were right, no theory could ever be refuted, or even weakened, by adverse empirical evidence. The adverse evidence could always be written off as just a 'queer coincidence'—inexplicable but needing no explanation. It does not follow either that if the

'Aristotelian' theory were refuted by empirical evidence, the 'Platonic' theory must be true. There may be other alternatives. For example, the Buddhist analysis of human nature is neither Platonic nor Aristotelian.

In the last page or two of his paper, Mr Robinson takes back his admissions that the coincidences collected by psychical researchers are at any rate queer. On the contrary, the emotional aura which they have for him is 'boredom and banality'. An alleged communication from Henry Sidgwick about the relation of mind and body strikes him as 'here and there ridiculous but otherwise a dead bore' (p. 204). To him, the reports of psychical researchers are like dead clichés or pointless schoolboy jokes. He cannot go to five tea parties without hearing at least one 'super-normal' story—fully authenticated, of course. In his view the trouble with the data of psychical research is not that they are too strange and queer to be believed; on the contrary, they are so insufferably tedious that one can hardly give one's attention to them.

I think that this sort of emotional reaction is not at all uncommon, especially among highly-educated people; and psychical researchers should give more thought to it than they have, if they want to understand and remove the 'resistance' which their data often meet with. Indeed, they would do well to read the whole of Mr Robinson's paper with care. It is a very good statement of views which are widely held, but not nowadays often expressed.

Mr Mundle's paper is very interesting indeed. But it is also very long and cannot be adequately summarised in a paragraph or two. I shall confine myself to four of the most important points in it.

First, when we ask whether psychical research is relevant to philosophy, what do we mean by 'psychical research'? According to Mr Mundle, we may mean (1) the observational data, or (2) the primary hypotheses, or (3) the speculative theories which have been suggested for co-ordinating the primary hypotheses. If we mean the observational data, he thinks that these *can* be described in Behaviouristic terminology, and indeed, must be. The experimenter's data consist in 'the percipient's overt behaviour and the written record produced thereby' (p. 209). And even in spontaneous cases, the private experiences of the percipient are not evidence, unless they result in some kind of overt action which can be confirmed by witnesses or documents. 'Psychical researchers accept as evidence only what Behaviourists can acknowledge as hard facts' (p. 209). I should have thought, on the contrary, that in the spontaneous cases the percipient's

overt actions are only of interest because they tend to show that the *private* experiences did, in fact, occur at or about the time when the percipient says they did.

Secondly, with regard to the relation of mind and body, some philosophers think that psi-phenomena, if they prove nothing else, do at least prove that Epiphenomenalism is false. Mr Mundle does not agree. For example, what we take to be telepathy may be a causal transaction between a *brain* event in the agent and a mental event in the percipient (it would then be a sort of clairvoyance). And surely this account of the matter is compatible with Epiphenomenalism? I think that it is only compatible with a new and extended version of Epiphenomenalism. For in ordinary Epiphenomenalism the cause of a mental event in Mr A. is an event in Mr A.'s own brain, not in someone else's. In any case, it is hard to see how precognition could be compatible with any form of Epiphenomenalism. And clairvoyance of the ordinary kind, where the 'object' is not a brain event but some non-physiological entity like a Zener card or a sealed-up letter, would not be compatible with it either.

Thirdly, Mr Mundle asks us to consider a new way of classifying psi-phenomena, though he does not ask us to accept it. In the new classification we abandon the terms 'mental' and 'physical', and replace them by 'human' and 'non-human' respectively. Then we have (1) non-chance relations between human events and other human events in the same or in different persons; (2) non-chance relations between human events and non-human events. The first head would cover 'inter-personal' PK, as well as telepathy. The second would cover ordinary PK, clairvoyance, and poltergeists.

This new classification would no doubt save us from begging questions about the relations between mind and body, and would please the Behaviourists whom Mr Mundle is so anxious to conciliate. But there are disadvantages in it too. Precognition would fall partly under the first head and partly under the second; and the most important thing about it—namely, its reference to the future—would tend to escape our notice. Again, the Survivalist explanations of mediumistic communications would be ruled out by definition, unless events in discarnate minds are counted as 'human events'; and if they were, the advantage Mr Mundle hopes to gain, of conciliating Behaviourists and other 'tough-minded' thinkers, would be lost. Moreover, there is some evidence that ESP occurs occasionally in non-human animals; and this would be ruled out also.

Fourthly, Mr Mundle distinguishes between two ways of interpreting psi-phenomena of the 'mental' kind (to return to the

traditional classification). In the one, the fundamental concept is *causation*; in the other, it is *cognition*. If we accept the causal interpretation, he thinks we shall have to re-define the term 'causal connection'. At any rate, we must *not* so define it as to make spatio-temporal contiguity part of its meaning, nor in such a way that the proposition 'a cause precedes its effect' is analytic. Moreover, 'is happening' must not be so defined that it is equivalent to 'is coming into existence'. For in precognition (on the causal interpretation) a future event has effects. It must therefore be in existence, though it is not yet happening.

Mr Mundle then points out that psychical researchers *have* produced explanations of the queer occurrences which they study, despite Mr Robinson's denials. They have done it, as other scientists do it, by postulating 'interphenomena' to make the observed phenomena intelligible. Thus they try to explain telepathy by the theory of a Common Unconscious, i.e. by postulating mental interphenomena; and they conceive the Common Unconscious as an inter-personally effective store of traces or engrams. Mr Mundle does not think much of this explanation. He objects that there is no reason to call this inter-personal, trace-bearing medium 'mental'. Perhaps not. But if it is physical, it will not do what is wanted, unless we go back to a Radiation Theory of telepathy, which Mr Mundle has already rejected (pp. 221-2). I think he has ignored another relevant point. It may be argued that an unconscious of some kind has to be postulated in any case, in order to explain the data of abnormal psychology; and we call it 'mental', *faute de mieux*, because our explanation will only work if processes analogous to conscious planning are supposed to go on in it. If so, it is economical to make use of the postulated unconscious for explaining psi-phenomena too (since we have to put up with an unconscious in any case); and we can do this by supposing that processes in the unconscious are inter-personal. Moreover, the Freudian mechanisms of repression and symbolic distortion are ready to our hands for explaining the queer and devious ways in which psi-contents manifest themselves in consciousness and in behaviour.

However this may be, Mr Mundle thinks we may do better if we interpret psi-phenomena cognitively instead of causally. If cognitive relations really do span spatial and temporal gaps, as philosophers suppose, will not this give us just what we want? The cognitions we should have to postulate would be themselves uncognized (i.e. subliminal); so they too would be 'interphenomena'. I am not sure that this cognitive explanation really differs so much from the causal explanation which Mr Mundle has rejected. Once we descend below the conscious level—as we

have to, on both theories alike—the distinction between ‘being an awareness of’ and ‘being an effect of’ seems to lose much of its sharpness; and the lower we descend (if this spatial metaphor is appropriate) the less sharp the distinction becomes. Moreover, if we apply the ‘cognitive’ theory to telepathy, we shall have to allow that there is such a thing as unconscious *inspection* of the contents of another mind; and this surely is something for which we can find no analogy in the sphere of conscious cognition.

Mr Mundle leaves it to the audience to decide whether these issues are among the ones which philosophers ought to discuss. But he himself clearly thinks that the answer is ‘yes’; and he is clearly right.

H. H. PRICE

HARRY PRICE: THE BIOGRAPHY OF A GHOST-HUNTER. By Paul Tabori. London, Athenaeum Press, 1950. viii, 316 pp. 11 plates. 15s.

Harry Price's long and varied career as an investigator of the physical phenomena of psychical research deserved a biographical record, and the choice of Mr Tabori to compile it was a happy one. In his skilled hands a great number and variety of incidents are described in an interesting way. He lays proper stress on Harry Price's wide reading, unflagging energy, mechanical skill, and knowledge of methods of deception, but he makes it clear that he is not blind to some peculiarities of temperament which brought Harry Price into conflict not only with persons whose views of psychic phenomena were opposed to his own, but with others who shared in general his opinions and interests and were anxious to co-operate with him. In fact, for the official biography of a man recently dead, the book is remarkably and commendably frank.

The book is very largely based on ‘the vast correspondence he [Harry Price] carried on, much of it unpublished and unpublishable’, but for an informed judgment on some of the controversial matters dealt with by Mr Tabori there are other documents that the reader would be well advised to consult. For example, as to Harry Price's proposals for the amalgamation of the self-styled ‘National Laboratory of Psychical Research’ with the S.P.R., he should read our *Journal*, vol. XXVII, p. 25, and for the much debated ‘exposure’ of Rudi Schneider he should put side by side the letters to Rudi printed on pp. 113 and 116 of this book with the facts stated by Lord Charles Hope in S.P.R. *Proceedings*, Vol. XLI at pp. 284–91. We regret that Mr Tabori appears to endorse Harry Price's preposterous claims to have contributed by

his researches to the foundation of the Perrott Studentship at Trinity College, Cambridge, and the acceptance by New College, Oxford, of the reversion of the Blennerhassett Trust.

Mr Tabori pays a graceful tribute to the help he received in preparing the book from Mrs Goldney and Dr Dingwall, who are familiar with many of the events related in the book. On some of these, particularly the Borley case, a more complete report based on a wider range of evidence than seems to have been accessible to Mr Tabori would be welcome. May we hope that they will see their way to provide it?

W. H. S.

WILLIAM JAMES: A SELECTION FROM HIS WRITINGS. Edited with a commentary by Margaret Knight. Harmondsworth, Penguin Books, 1950. 248 pp. 1s. 6d.

Members of the Society for Psychical Research will be interested to read this book on our distinguished former president, Professor William James. James was a vivid and self-revealing writer and the greater part of the book is composed of judiciously chosen extracts from his own writings. These are preceded by a biographical account which helps one to understand the family situation that made James the intellectual adventurer that he was. While the extracts are mostly from his psychological writings and not from any contributions he made to psychical research, the biographical sketch and the writings make clear why he was attracted to psychical research. Born into an argumentative family, he was always impatient of accepted orthodoxies and had a quick eye for the unexpected fact. That psychical research did not fit into the scheme of the biological psychology which he himself taught was for him a reason for being interested in it rather than ignoring it as a more conventionally minded man might have done. As in his study of religion in his Gifford Lectures, he brought to bear on psychical research an inquiring and open mind with a tendency to scepticism which led him to question the dogmas of orthodox science no less than any new and unexpected phenomena.

R. H. THOULESS

ENTHUSIASM: A Chapter in the History of Religion with special reference to the XVII and XVIII Centuries. By R. A. Knox. Oxford, Clarendon Press, 1950. viii, 622 pp. 30s.

'When strong currents of spiritual emotion, aroused by some religious crisis, sweep through a multitude of human hearts,

physical reactions of an abnormal kind are liable to occur as their byproduct' writes Mgr Knox in this learned and stimulating study of fanatical sectarianism over eighteen centuries. The physical reactions displayed in some of the revivalist movements chronicled are, indeed, odd enough; men and women are shown expressing their sentiments by leaping, shaking, rolling, trembling, talking gibberish, swelling up, and (my own favourite) 'barking demurely' at those who preached to them.

As bizarre, and of greater interest to psychical research, are the borderline phenomena which sometimes accompanied these manifestations. The most notable are perhaps those seen in the prophesying of the Camisard Huguenots after the Revocation of the Edict of Nantes; and those of the Jansenist convulsionaries of Saint-Médard, of which Dr Dingwall has written a short account.<sup>1</sup>

A particularly fascinating point about the earlier movement is that its 'prophets' were apparently taught some sort of psychophysical technique. 'M. du Serre . . . collected fifteen children of either sex from the peasantry of the Vivarais, and trained them in the art of prophecy. His school was at Mont Péyra in the Dauphiné . . . he initiated his pupils, exercised by two or three days' fasting, into the four grades of prophecy' which were known as 'l'Avertissement, le Souffle, la Prophétie and le Don'. Those interested in the natural history of contemplation may find it significant that a pupil who had reached the stage of 'le Don' frequently abandoned his former preaching activities; activities whose normal pattern ran thus: 'The prophet beat his head with his hands for some time, then fell down on his back' (sometimes accompanied by his more impressionable congregations) 'his stomach and throat swelled up, and he remained speechless' for some time, then 'broke out into utterance.'

The five or six hundred children between three and fifteen who were trained at this school seem, however, to have shewn more interesting results than this; there is evidence that 'some drove knives into themselves', exposed themselves to fire, and fell from rocks twelve feet high without being injured, while others shewed that they possessed extrasensory powers, narrating events which were taking place at a distance, and accurately foretelling the future. The whole episode, with its apparently deliberate organization and training of children below the age of puberty, is curiously reminiscent of what is said about the psychical techniques of the East.

The Saint-Médard affair has a more spontaneous ring. It began quite simply with the cure of a number of sick people at the tomb of the Jansenist ascetic, François de Pâris. Then, after a convul-

<sup>1</sup> *Some Human Oddities*. London, Home & Van Thal, 1947.

sion had accompanied the cure of a paralytic girl, convulsions became the general rule. Mgr Knox points out that they were at first regarded as the concomitants of healing; but that quite soon their occurrence became an end in itself, as an evidence of the miraculous. Later still came the *secours*; this was at first regarded as a counter-irritant to the pain of the healing convulsion; then and with greater reason, it too was held as proof that the supernatural was at work. The *secours*, for which the convulsionaries might crave as a source of relief, took a variety of forms. The sufferer might ask to be trodden on; to be subjected to the pressure of heavy weights; to be beaten; to be prodded by sharp objects, such as swords or spits (which, curiously enough, very often pierced the clothes, but never the skin); or to be crucified.

The evidence for these occurrences is very good, in so far it was collected and recorded by persons who would much rather have believed them to be faked; for the logical justification of these 'miracles' was to provide support for the Jansenist teachings, and to protest against the enforcement of the Bull '*Unigenitus*' which condemned these; and they were chronicled by those who held the opposite view. Here again, some of the actual phenomena—the rotation of sword blades within the sockets of a sufferer's eye, the prolonged 'whirling' of other sufferers, the insensibility to pain—recall stories of Eastern fakirs, and of the dancing dervishes of North Africa. It may also be remarked that, as in the case of Rasputin, some of the healings later performed had little to do with the personal sanctity of the healer; thus, for instance, the numbers of cures of ulcers and abscesses attributed to one particular woman shewed no diminution when she took to sexual irregularity. Healing is obviously a less demanding occupation than lion-taming, for which chastity is essential.

RENÉE HAYNES

HYPNOTISM AND THE POWER WITHIN. By S. J. van Pelt, M.B., B.S. London, Skeffington, 1950. 208 pp. 18 plates. 18s.

Dr van Pelt, President of the British Society of Medical Hypnotists, has written an account, clearly intended for the general reader, of hypnotism and its use in the treatment of neurotic ailments. He has also taken the opportunity to deliver an attack on psycho-analysis, prefrontal leucotomy, electric convulsion therapy and other methods of shock treatment, Christian Science, and stage hypnotism. Except to express some surprise at the violence of his offensive—especially on the founders of psycho-analytical theory, to whom he denies credit for any contribution to our knowledge—we are not concerned with these matters here.

Dr van Pelt devotes several pages to the argument that the mediumistic trance is identical with the hypnotic trance. Although the two states have many features in common, it would be premature to say that they are identical. Some evidence pointing to a difference between them has been provided by the electroencephalograph. In the hypnotic trance, the E.E.G. shows the same pattern as the waking state. Experiments carried out by J. L. Franke, a neurologist of Haarlem (see *Tijdschrift voor Parapsychologie*, 1938, 10, 111-17), led him to the conclusion that the brain appears to be 'asleep' in the mediumistic trance. It is also of interest that Thomson, Forbes, and Bolles, with a subject who was able voluntarily to induce light trance states in himself, found an E.E.G. which was characteristic of light sleep (see *Amer. J. Psychiat.*, 1937, 93, 1313-14). The E.E.G. was, however, then in an early stage of development, and there is need to repeat this work with modern and more reliable models. The present writer has been unable to trace any more recent attempt to obtain the objective evidence of the E.E.G. on this point.

Intending readers should be warned that the book is exasperatingly repetitive, and that (in common with so many other works published by the firm of Hutchinson and its associated companies) there is no index.

E. O.

DIANETICS: THE MODERN SCIENCE OF MENTAL HEALTH. By L. Ron Hubbard. New York, Hermitage House, 1950. xxvii, 452 pp. \$5.00.

This book is certainly a work of genius. The author claims to have discovered, by following the Scientific Method, not only how the mind works in health but also the one true cause of all psychological illness. He presents a new method of psychotherapy which he guarantees will cure all cases of such illness without fail, and will further transform normal man into superman.

The genius of the author lies, however, not in the painstaking and accurate manner in which he has accumulated, sorted, and presented his evidence, for there is none—not one scrap; nor in his powers of invention and salesmanship, magnificent though they are (his self-assurance and arrogance are superb); but it is in the fact that he has been able to produce a system of psychology based on the most obvious untruths which has nevertheless, in America, competed with and to a large extent defeated all rival systems of redemption. The author must have realized that people in these times will listen to any Messiah who claims complete authority in the name of Science and will relieve them of the

burden of being morally responsible for their actions. Mr Hubbard's system has the further advantages of undercutting its rivals in cost, and in the fact that anyone can practise it. The sole qualification seems to be the purchase of Mr Hubbard's book; this of course saves the tiresome business of an expensive medical training.

Dianetics consists of: (i) rewriting a lot of platitudes in brand new jargon ('Aberree' is a good example); (ii) large quantities of Freud rewritten in a mixture of current teen-age American and electronic terminology; (iii) the crib of the engram.

The basic idea in dianetics is that the mind consists of two sections: (a) the Analytic Mind which works as a perfect computing machine; and (b) the Reactive Mind, a jazzed-up version of that well-worn vehicle, the unconscious, which causes all our troubles by interfering with (a). It appears that whenever we are unconscious, asleep, injured, drugged, or *in utero*, the 'reactive' mind is wide awake, picks up all painful stimuli, and stores them as impressions on the actual protoplasm of the brain cells. These impressions are the engrams, and cause the bulk of our troubles from ulcers to war. Dianetic therapy exorcises them, and superman, the perfect analytical machine, is produced. The patient is relieved of all responsibility for his actions since he can either blame the engrams or function happily in the iron determinism of a perfect machine.

The technique of Dianetic Therapy, though this fact is heavily disguised, consists of powerful suggestion applied during a light hypnotic trance to a gullible subject. The subject, having had the theory of engrams explained to him, obligingly 'recalls' infantile or pre-natal experiences, with cathartic accompaniment.

Mr Hubbard's dismissal of ESP as an explanation of the alleged ability of dianetic therapy to recall pre-natal experiences is, of course, otiose. Indeed, his statement that one 'should not accept telepathy any more than he would accept ESP' (p. 321) to account for the 'phenomena' is a measure of the half-knowledge and confused thinking which pervades the book.

Dianetics is the Mumbojumbology of Dr Rhubarb come to life. Mr Hubbard might well have taken his cue from the following extract from Beachcomber's column published in the *Daily Express* a few months ago:

One or two people have asked what is meant by Mumbojumbology, of which Dr Rhubarb is the foremost living exponent. It means idiotic veneration. The Abracadabrists, led by Bottelburg, discovered that any drivel uttered repeatedly, and in an authoritative tone, will be accepted today with the fervour formerly accorded to the worship of an African idol. But Dr Rhubarb widened the appeal of the new philo-

sophy by relying less on completely meaningless sentences than Abracadabraism had done. He evolved a system of half-meanings uttered in a jargon of bastard Latin and Greek words. He also concentrated on clothing obvious lies in pseudo-scientific gibberish.

The depressing aspect of the whole shameful affair is that half a million Americans, some of whom are sincere and intelligent people, have fallen for it.

J. R. SMYTHIES

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 45, No. 1, January 1951. New York, A.S.P.R., \$1.50.

Mrs Lydia Allison contributes an obituary of Mrs Leonora Piper, the well-known 'mental' medium who was at the height of her powers in the eighteen-nineties. Dorothy A. Berg in a useful review compares and contrasts various theories that have been put forward concerning the *modus operandi* of trance communication.

#### WATER DIVINING

The names L. A. Dale and Gardner Murphy at the head of a report guarantee a carefully designed research project. They have collaborated with R. M. Greene, W. Miles, J. M. Trefethen (Professor of Geology, University of Maine), and M. Ullman, M.D., in 'Dowsing: A Field Experiment in Water Divining' (pp. 3-16). It is, as far as I know, the first adequately planned and supervised field experiment in which dowsers and professional geologists have been matched on equal terms. It was *not* the purpose of the experimenters 'to present an evaluation of the evidence for and against the general claims of water diviners or of the conclusions reached by other investigators. . . . We should not wish to be interpreted as believing that we have contributed any findings which radically change the overall research situation'.

The problem which they set themselves was simply: 'Can water diviners find water under conditions in which the professional geologist is unable to do so?' It was necessary to choose a level terrain without surface water or wells which would betray the presence of underground water, and in which digging or drilling could be used without great expense to check the results. A search led to the selection of a reasonably satisfactory tract of land near Liberty, Maine, and an advertisement was inserted in four Maine newspapers.

WATER DIVINERS—Dowsers, those who can find water with forked stick, are invited to take part in search for water in open country near Liberty, Maine, on August 4th, 5th, or 6th [1949] for the benefit of scientists

interested in this power. Travel expenses and \$12 for the working day available ; also prize for outstanding success. Those interested write to : Gardner Murphy, Ashland, N.H.

Twenty-seven dowsers were finally selected (22 men, 4 women, 1 adolescent girl). Details of the life history and of the dowsing history of each were noted. At the close of the interview the diviner was conducted to a spot near but out of sight of the test ground, where he was blindfolded. Most of the diviners brought their own sticks or rods with them, and it is interesting to note that not one of them subscribed to a psychic or psychokinetic theory of dowsing ; all believed it to be an as yet unexplained *physical* phenomenon.

Each dowser was led over the site first with blindfold and then without. Records were taken of his method of working, and of the number, intensity, and form of the rod-movements. As a rule, there were several points with blindfold and several without at which the rod turned and an attempt was made to get the dowser to specify depth and rate of flow at each point ; most were very reluctant to specify rate of flow except vaguely as 'good' or 'poor'. The dowser was asked to name his 'best' spot chosen with blindfold and his 'best' without blindfold, and for these two spots a quantitative estimate of rate of flow and depth was insisted on.

Subsequently Trefethen, the Professor of Geology, and a local water engineer made their own estimates of the depth and rate of flow at sixteen pre-arranged points. The experts quite properly made use of their knowledge of the existence of a nearby well: '.... knowing that the water in soft soil must be nearly level, they could properly apply their information. The dowsers, thinking and working in terms of "veins" of water (which did not of course exist) could hardly make use of the fact of the well even had they known about it'.

Then pipes were driven down to determine the depth, and rate of flow was estimated by pumping timed with a stop-watch. This was done (with certain unavoidable exceptions) at each indicated point. The results are illustrated graphically, as 'scatter' diagrams. In the case of the dowsers there was no general correspondence between the estimates and the actual facts. Both depth and rate of flow were greatly overestimated. For example, a blindfolded dowser estimated 75 gallons per minute when the actual flow was 1 gallon per minute, while another gave 25 ft. where the depth was 4 ft. The two 'experts' on the other hand estimated quite closely the over-all depth of the water table and the depth at specific points, and also agreed with each other. The correlations were mathematically significant. As to rates of

flow, 'the water engineer did well in over-all estimate of *rate of flow*, but did not appropriately vary his estimates from point to point within the terrain; the geologist's estimates of rate of flow were too low and likewise unrelated to the actual point-by-point variations'.

There was some discussion with the dowzers about the 'artificiality' of the conditions, since it was clear that nobody was planning a house or well on that site. But if a test of this kind is not a valid test of dowsing then we may as well throw up the sponge and declare that the phenomenon, as claimed, is untestable. If the conditions are accepted, and *if* the results of Dale *et al.* are confirmed, we may, rather reluctantly, have to label dowsing as a spurious phenomenon.

Meanwhile, the report concludes: 'But two major issues remain open: (1) Does water divining ever occur by virtue of a paranormal process? and (2) Is the motion of the stick invariably the result of the diviner's own muscular contractions? The present writers do not wish to commit themselves, . . . believing that far more research is needed before they can be answered.'

D. P.

## CORRESPONDENCE

### THE STATISTICAL EVALUATION OF GROUP EXPERIMENTS

SIR,—Dr Schmeidler's interpretation of her interesting and important work reported in the November–December *Journal* seems to be marred by a statistical error. She appears to have treated group experiments, in which several percipients guess at the same target, by the same methods as she would treat individual experiments, in which every percipient has his own run of targets.

The mistake is important and it has occurred before. What Dr Schmeidler has overlooked is that the statistics of card-guessing must be based on the question: given the guesses, how likely is it that the targets would have corresponded with them at least as closely as they do? and not, given the *targets*, how likely is it that the *guesses*, etc.? This is because we know the targets to be random, but we do not know about the randomness of the guesses. It happens that if there is only one percipient per target the statistics are the same whichever question we ask, and that is perhaps why this point has often been overlooked. But where several percipients guess at the same target it can make a great deal of difference.

A more succinct but less instructive way of putting essentially the same criticism is that we cannot evaluate the results of several subjects independently since their results are not in fact independent; but this does not bring out the essential point that, since the statistics must treat the *targets* as variable and the *guesses* as given, we cannot expect to get more significant results by using more percipients to guess at the same targets. From this fact it is clear that the only advantage to be expected of group experiments is that they should be more representative and perhaps more consistent.

How can group experiments be evaluated? In theory by the use of the elaborate multiple matching methods developed by Greville and others, but in practice more simply by the almost equivalent method of getting a group opinion—e.g. a majority opinion—on each guess. Thus, if the most popular guess for a given exposure of a target is Star, the 'group-guess' may be taken as Star, and the group is then treated statistically as an individual. The group opinion can be determined either at the time of the guess or after the session. Both methods demand extra time and work, though much of this can be saved by a resourceful experimenter.

What about Dr Schmeidler's experiments? I understand that the 1943-45 series (sheep *v.* goats) was individual and the rest (sheep *v.* goats with Rorschach studies) group testing. The former were therefore correctly evaluated, and since the results were significant it seems reasonable to take at their face value at least the sheep *v.* goats part of the group studies. To clinch the matter all we need to know is: in how many runs the mean sheep-score exceeded the mean goat-score and in how many the mean goat-score exceeded the mean sheep-score; and similarly for good and poor adjustment, etc. It seems likely that the results would still be significant. I wonder if Dr Schmeidler can give them to us.

I would like to express the hope, too, that in any future reports Dr Schmeidler will state explicitly whether each result comes from individual or group experiments, and give rather more complete accounts of the procedure used for statistical evaluation.

CHRISTOPHER SCOTT

SIR,—Mr Scott's criticism is technically justified. In the article to which he refers, probabilities were calculated from *t*-scores both for the individually tested subjects and for the group tests in which all members of a class guessed at the same targets. As he points out, this is not theoretically correct.

But does it matter, in practice? I should like to raise two points which indicate that it does not; and that the method which was used leads to the same conclusions as the correct but more time-consuming one devised by Dr Greville.

The first is empirical. In several other series which were similar in procedure and in the nature of the target, both the Greville and other methods were applied, and no appreciable differences were found. Dr Betty M. Humphrey states in relation to this:

In every case the CR of the difference obtained [by the Greville method] differed from that obtained by the binomial formula by only a few hundredths of a point. (For example, for Series S<sub>1</sub>, S<sub>2</sub>, and S<sub>3</sub>, CR's of the difference obtained by the binomial formula were 2.79, 2.81, 2.29. By Dr Greville's method the comparable CR's of the difference were 2.76, 2.75, 2.26.)

Because of the almost prohibitive amount of work involved in carrying out the extensive analyses according to the Greville method, and especially because of the fact that no appreciable difference has ever been found in connection with any of the series of this report between the results obtained by the Greville method and the simple binomial formula, it was deemed unnecessary to apply the Greville method to the series of the present report where the CR's obtained were not of borderline significance. (*J. Parapsychol.*, 1949, 13, p. 157, footnote.)

It will be remembered that the two probability figures quoted in my article were  $P = .000001$  for the difference between mean ESP scores for well-adjusted 'sheep' and well-adjusted 'goats'; and  $P = .0003$  for the difference between mean ESP scores of the sheep whose Rorschach protocols were free of seven 'signs' and of goats whose protocols were free of the same signs. Since neither of these figures would ordinarily be considered borderline, I followed Dr Humphrey's precedent in evaluating the data. Even allowing for some margin or error because of the statistical method used, we can, I think, take it that differences between the groups were demonstrated by the research. If this is so, spending many hours on further evaluation would not contribute to our understanding.

My second point relates to theory. The reason for applying a correction to multiple calls on a target is the possibility that many members of a group have a preferred pattern of symbol choice, which (if it exists) may be similar to, or different from the symbol pattern of the target list. This can, of course, affect the data when an experiment uses only one target list. In our case, however, 27 groups guessed at 9 lists each, making 243 lists in all. It seems obvious that the importance of multiple calling is minimized when so many sets of multiple calls are used.

In this connection, Dr Greville has written to me, 'Experience

has shown that similar stimulus preferences among the members of a group are an important factor only when (1) the number of possible stimuli from which the target is selected is very small, and (2) the number of calls in an experimental session is also very small.'

Neither of these conditions was fulfilled in the research to which Mr Scott refers; thus his criticism would seem to be inappropriate.

There is one comment of Mr Scott's which should not go unchallenged. In his third paragraph he writes, 'We cannot evaluate the results of several subjects independently since their results *are not in fact* independent.' (The italics are mine.) Here he assumes a point which he might find very hard to demonstrate. The argument against using *t*-scores for multiple calling of a target is based on the assumption that members of a group have similar stimulus preferences; but this is only an assumption. Examination of the data shows that any such similarity is so small that it is not perceptible to ordinary observation. This is also demonstrated by the fact that Dr Humphrey found such small differences between CRs obtained by the Greville method and the binomial formula.

The general problem is one to which many of us in psychical research are especially sensitized. Often, in every field but particularly in ours, critics point out, correctly, possible loopholes in the procedure of some research. These criticisms are valuable for later tightening of the procedure and proper evaluation of the data. But if the critic assumes that what *might* have affected the results did 'in fact' affect them, he will be making the same sort of error as the experimenter who assumed that what might have affected the results did not affect them at all. Sometimes, as a result of such irresponsible criticism, worthwhile or provocative research is dismissed too readily as worthless. (This generalization is not intended to apply to the present case, nor to Mr Scott's courteous letter.)

GERTRUDE SCHMEIDLER

#### 'RETROACTIVE PK' AND THE CLASSIFICATION OF PHENOMENA

SIR,—I should like to make two comments on the interesting letter from Miss Pamela Clark which appeared in the *Journal* (January–February 1951) and in which she referred to my paper in Part 178 of *Proceedings*.

When Miss Clark says that I recognize 'two main groups of

phenomena, one cognitive and the other conative', she attributes to me a view which I have never wished to defend. Miss Clark mentions my criticism of the term 'precognition' on the grounds that it begs (or seems to beg) the question whether the facts in question are attributable to cognitive acts. The term 'extra-sensory perception' is, of course, open to the same objection. I agree with Miss Clark's statement that 'there may be something unsatisfactory for psychical research in adopting without question our everyday categories of cognition and conation'. Indeed, I intended to underline this point when I argued (on p. 72 of my paper) that we are not entitled to regard the introspectible difference between 'guessing' and 'willing', as they appear to the subject, as a reliable criterion for classifying psi-phenomena.

My second point concerns Miss Clark's suggestion that we should define phenomena 'as far as possible in positivist terms'. It may be worth mentioning that this policy, if rigorously applied, would, I think, render it *meaningless* to distinguish 'retroactive PK' from 'retrocognitive clairvoyance'. The distinction between these concepts surely hinges on our interpreting causal connection between a pair of events, *A* and *B*, as a relation which holds in a certain direction (as an asymmetrical relation), so that there is a difference in meaning between saying '*A* causes *B*' and '*B* causes *A*'. The direction of causal influence between two events is not, however, something which can be observed by the senses; hence a positivist should deny that this is a legitimate or useful feature of our concept of causation. The common-sense belief that causes must precede their effects would be attributed by most positivists to an arbitrary linguistic convention concerning our use of the word 'cause'. (That this is not the whole story is illustrated in the discussion by Professors C. D. Broad and H. H. Price of 'The Philosophical Implications of Foreknowledge' in *Proceedings of the Aristotelian Society*, Supplementary Volume XVI.)

It does appear tempting, in view of the temporal displacement effects found in psi-phenomena, to abandon the conception of causation as an asymmetrical relation. The main attraction of this course is that it seems to dispose of the great theoretical difficulties involved in supposing that events may be directly influenced by events which have not yet happened—difficulties which would apply equally to cases of 'retroactive PK' and cases of 'precognition', and which seem to be due primarily to our taking causal connection to be an asymmetrical relation. If this course were adopted, it would require some radical changes in the classification of psi-phenomena. It would, for example, imply (as I argued on p. 63 of my paper) that PK and precognition are not independent

concepts. I wonder whether Miss Clark is prepared to be a consistent positivist?

C. W. K. MUNDLE

### ERRATUM

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In Table IV on page 365 the figure 134 should be 74.